

Sputnik + 30

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When Sputnik was launched on October 4, 1957, I was in Melbourne, Australia, a Fulbright scholar from the University of Wisconsin, visiting MacFarlane Burnet's laboratory. In the Southern hemisphere, we could promptly observe it with our own eyes. That winter had been notable for the brilliance of the Aurora Australis—the more reason for celestial observation. Of course, the event prompted intense excitement about its scientific-technological as well as military-political implications.

A month later, November 6, 1957, I arrived in Calcutta to visit J B S Haldane. He had only recently left England and established himself at the Indian Statistical Institute “as a refugee from the US occupation of Britain”. That day was the occasion of a lunar eclipse, with religious processions in the crowded streets.

The eclipse was the main topic of conversation at dinner: Haldane remarked that this was the 40th anniversary of the “October” revolution: it might be a second coup, after Sputnik, were the Russians to plant a red star on the moon during the eclipse! We calculated that a thermonuclear demonstration, accenting the military prowess signified by Sputnik, might indeed be visible from earth. It was depressing to me that we had even to contemplate the possibility. Our political views diverged sharply, but we shared the lament that this magnificent scientific opportunity, the beginning of human exploration of space, would likely be marred by the geopolitical competition, that it would be used for propaganda demonstration rather than scientific inquiry. Furthermore, we might have to take measures to protect the moon and other planets from inadvertent radioactive or biological contamination arising as byproducts of the circus.

Since childhood, I had been intrigued by the scientific debate over the possibilities of extra-terrestrial life. As a thirteen-year old I had listened with amusement to Orson Welles' notorious radio broadcast modelled on H G Wells' novel, *War of the Worlds*. I had thought that the subsequent news reports of public panic were part of the Halloween spoof itself! Twenty years later, my main professional work, on the genetics of microbes, inevitably focussed my interest in the ultimate origins of life. The possibility of its divergent evolution elsewhere than on our own planet was self-evidently one of the most important challenges to biological science. The tools to meet them were finally in our grasp.

Promptly after returning to Madison I steeped myself more deeply in the general physical and astronomical background of space inquiry, and of rocketry and space travel. In December 1957 I composed the first of a series of memoranda. The first of these was entitled *Lunar Biology*, the second *Cosmic Microbiology*. I circulated these to a dozen or so scientific notables, asking their comment on what step might be taken to avert what I saw as a potential cosmic catastrophe, and inviting broader scientific examination. One of these communications was addressed to Detlev Bronk as the President of the National Academy of Sciences; another to Fred Seitz who was on its governing council. (Bronk at that time was also President of The Rockefeller Institute; Seitz was his successor and my predecessor in that role.) On February 8, 1958, the Council of the NAS formally expressed its concern about planetary contamination and asked ICSU to take appropriate action. ICSU did establish CETEX, an international committee on contamination by extraterrestrial exploration. It met in May 1958, and concluded "there is a real possibility that early experiments might spoil subsequent research", affirming the general principles of caution that I had outlined in my December memoranda. In remarkably short time, a sound basis of policy examination had been established.

During the succeeding months US space activities were reorganized under a civilian agency, NASA. The administrator, Hugh Dryden, asked the NAS to establish an advisory Space Science Board, chaired by Lloyd Berkner. He in turn asked me to chair a panel to study the problems of planetary quarantine, and biological scientific opportunities in space travel. Other panels examined the problems of life support systems and the physiological stresses of the space environment.

In order to minimize transcontinental (terrestrial) travel I suggested to Berkner that two, small parallel groups be established, on the west and east coasts respectively. The "western group on planetary biology" (WESTEX) was held at Stanford on February 21, 1959, a few weeks after I had moved there from Wisconsin. (In the interval I had been burdened by an unexpected trip, namely to Stockholm for the Nobel Prize.) It was a lustrous group, including among others: Melvin Calvin, Norman Horowitz, Dan Mazia, Matt Meselson, Aaron Novick, Roger Stanier, Gunther Stent, Harold Urey, C B van Niel and Harold Weaver – the last being the one professional astronomer. Likewise, the eastern division met on December 19, 1958, at MIT. Salvador Luria played a substantial role in that group, as well as Paul Doty, Tom Gold, Keffer Hartline, Martin Kamen, Cy Levinthal, Stanley Miller, F O Schmitt, and Wolf Vishniac. (Wolf we lament as a casualty-in-action: he died in 1973 of an accident in Antarctica whilst conducting microbiological field surveys related to our collective experiments.) Subsequently, I was invited to join the NAS Space Science Board and take part in broader deliberations about space policy: this gave me a stronger but never very effective voice to urge that scientific enquiry predominate in the policy rationale for investment in space technology.

My first preoccupations had been about planetary conservation, to protect the opportunity for investigating their virgin surfaces until the technical possibility emerged. The US frantic efforts to emulate Sputnik in orbital flight succeeded only on January 31, 1958; and it seemed premature to many to be contemplating lunar, much less planetary, landings. This was precisely my concern: that early approaches to the moon or planets would be crude crash landings most likely to result in contamination, e.g. from radio-isotope electric generators.

In my meetings with various NASA representatives and at JPL, Al Hibbs and others put it to me that I should be undertaking a constructive as well as critical role: why didn't I take a positive part in the development of biological instrumentation for space exploration? Accordingly we established an instrumentation research laboratory in December 1959 with preliminary support from The Rockefeller Foundation, and with a definitive grant from NASA to Stanford in April 1960. I was fortunate to be able to recruit Elliott Levinthal to undertake the directorship of that laboratory and begin our experimental program in "Cytochemical Studies of Planetary Microorganisms". That was an arch title: the only planetary organisms we had were terrestrial ones. But we pondered the methodology by which life might be most efficiently sensed by instruments on a lander on Mars. My implicit assumption was an automated unmanned mission. The question is not really whether human intelligence should play a part in space exploration, but whether it is more effective in ground control stations or in the spacecraft—where the human presence imposes enormous logistic costs, and the imperative of return flight. The controversy continues to the present day.

At around this time, I coined the term "exobiology", a smaller mouthful than "the scientific study of extraterrestrial life". Exobiology has been panned as one of the few scientific disciplines that may have an empty set as its experimental objects. Regardless, what we have called biology until now should be limned "esobiology", which can be backformed into "earth's own biology". It may be unique in the solar system, perhaps even the cosmos—howbeit, it is still parochial.

One of our early speculative papers (with the late Dean B Cowie) suggested that the organic matrix of life might be cosmological rather than eso-atmospheric in origin, the latter being the prevalent Oparin-Urey-Miller model. We were wrong in suggesting that the moon's surface would be a likely place to look for unweathered meteoritic infall. It has been gratifying to note the accumulation of microwave astronomical observations of interstellar organic matter, capped by the recent analyses of Halley's Comet. We should warn against confusing these discussions of sources of organic molecules with the claims of Fred Hoyle and N C Wickramasinghe about bacteria in space!

Another of our discoveries was Carl Sagan, just completing his dissertation at the University of Chicago, whom we introduced to NASA in 1958 as an adviser to WESTEX.

We must pass quickly over the next two decades of my engagement with the NASA establishment, and its engineers' extraordinary technological achievement. Planetary travel was a reality far sooner than any of my scientific colleagues would have allowed. In October 1977, not one but two Viking landers (launched in 1975), were thriving on the Mars surface, but they were returning pictures and chemical analytic data of a bleak surface, rather discouraging (no trace of organic carbon according to the mass spectrometer) for any prospect of life.

Today, the most that one can say about a Martian exobiota is that a number of habitats on the planet, particularly at high latitudes, remain to be explored. Permafrosts probably do retain some moisture, and internal heat and chemical seepage arguably could support living organisms at some depth underground—not unlike the thermal vents on the floor of earth's oceans. Many large scale topographic features seem to signify ancient (if now desiccated) oceans and rivers, and these may bear fossils of a more hospitable epoch in Mars' history.

Science is still a poor cousin in the priorities of the nations' space programs. These remain the spectaculars of manned flight, at any cost in treasure and risk to life and constraint on other programs, including uncontroversial military needs. Perhaps this responds to the people's voice; but until they are altered, we will retain a constricted esobiological vision for many years to come.

Dr. Lederberg was a member of the Mars Viking Lander Experiment Team.